COVER SHEET

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PHILOSOPHICAL FOUNDATIONS OF BUILDING ECONOMICS: THE PROBLEM OF INDUCTION

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Summary

This paper considers the empiricist approach to the problem of induction and the implications in applied building economics. Induction, or the generalising from small to large samples, or generalising from past to the future, is shown to be logically indefensible as a legitimate means of knowledge acquisition thus bringing into question the whole of empirical research as a means of generating knowledge. This raises the issue of the distinction between knowledge and beliefs and the nature of probabilistic statements, and this is examined in the context of building economic modelling. It is concluded that probabilistic statements concern degrees of belief and should be tested as such.

Keywords: Building economics, philosophy, induction, empiricism, probability, models, knowledge, beliefs.

Introduction

"... the detection of a cost trend does not necessarily imply that it will continue in the future. Indeed it is very unwise to extrapolate a cost movement ..." (Ferry & Brandon, 1991:119).

"... it may be possible to extrapolate a trend into the future. However, there are very great dangers in attempting this" (Ferry & Brandon, 1991:130).

"Most samples are intended or believed to represent the population from which they were drawn. There is no way of making certain that this is so ..." (Beeston, 1983:8).

One of the most important tasks of building economists is to forecast future events of economic significance. In practice, this is done by a process of extrapolation from past events. Seemingly relevant data are analysed for trends, and these trends are assumed to extend into the future. The nature of scientific enquiry is essentially just this also. In this case the trends are taken to be governed by 'laws' (ie., the 'laws of nature'). Here the 'laws' are discovered by scientists by the well tried and tested process of rigorous experimentation and replication of physical phenomena. The laws are treated as causally based. The occurrence of one type of event (cause) is observed to be followed by the occurrence of another type of event (effect). When event, C, is **invariably** followed by event, E, it is intuitively obvious that C causes E[1].

[1]the relationship connecting the two events is termed a functional relationship. ie., E is a function of C,

Where the C is not invariably followed by E, but only 99% of the time, it is still relatively easy to imply cause and effect, the 1% residual usually being attributed to some type of experimental error. In this case the research chemist would assume the 1 in 100 'bad' result to be due to the use of impure chemicals. A similar approach tends to be taken in physics, where the classical assumption is that the lack of an absolute vacuum is responsible.

Where C is followed by E only 90% of the time, attributing cause and effect becomes rather less intuitive. At 50%, it is assumed that the research has failed and another line of investigation is started off on a different tack.

In social sciences research, and building economics is a social science, 100% relationships never occur. Many never even approach this desirable state. Thus, all social science research leads to failure by physical sciences standards.

What is to be made of this? Is social science, and therefore by implication, building economics research, a waste of time? Are we lacking in standards? What are our standards? What should our standards be?

The obvious argument is that a relationship, even if it holds only 50.001% of the time, is still an improvement on mere chance, which can only be expected to hold exactly 50% of the time. On this basis it is better to do something than nothing at all. But this ignores opportunity cost. The other side of the equation is that all research entails a cost and if the costs exceed the benefits to the extent that pursuing another activity has a better cost-benefit outcome, then it makes economic sense to pursue that other activity instead. There are two problems even with this though. The first is that the costs and benefits cannot usually be estimated very accurately. Thus we do not know the costs of the research until we have done it, neither do we know the of the existence of the miserably weak relationships until we have done the research. The second problem, and which actually subsumes the first, concerns the extent to which the estimated or even the true relationships observed can be assumed to continue into the future. This is the classical 'problem of induction' and to which this paper is aimed at addressing.

In the first part of this paper, the historical foundations of the problem of induction are examined via the British Empiricists school of philosophy. This is of necessity a fairly lengthy treatment in view of the general lack of awareness of the subject area among construction management researchers, and the exmples given are of a none technical nature to keep the flavour of the original ideas of the philosophers involved. In the second part, the implications for social sciences are considered in general

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denoted as E=f(C), with the implication that C 'causes' E.

and building economics in particular.

It might be argued that this paper presents an overly dogmatic attitude to existing systems and that a more sympathetic appraisal of existing techniques is needed. This may well be the case and it is to be hoped that such a view may be presented in some future paper on the subject. Meanwhile, however, the combined limitations of my knowledge and writing space have dictated events and what you see before you is the best I can muster thus far. The hope is that the reader will approach the paper with patience and trust. The intention is certainly not to be controversial nor even prescriptive. Instead, the aim is to lay the groundwork for future discussion on the subject by opening up a body of knowledge that is relevant to the foundations of building economics.

Historical perspective[2]

The problem of induction can be appreciated through a philosophical contrast between two kinds of argument, deductive and inductive. In the case of a valid deductive argument, the conclusion logically follows from its premise (eg., from the premises 'all As are B' and 'this is an A', the conclusion 'this is a B' follows logically). So if the premises are true, then the conclusion must be true and it would be self-contradictory to assert the premises and deny the conclusion[3].

In the case of an inductive argument, the conclusion does not logically follow from the premises (eg., from premises 'some As are B' and 'this is an A', the logical conclusion 'this is a B' does not follow because it would not be self contradictory to claim that every A experienced so far is a B but that the next A, or any future A, is not a B. However, although every inductive argument is invalid as a putative deductive argument, we feel that in the case the premises in some sense make it rational to accept the conclusion. Defining this sense is the problem of induction. The problem is sometimes called the 'justification of induction' because it requires us to justify the inference that 'all As are Bs' or 'this A is a B' from 'some As are Bs' where this does not follow logically.

One of the first people to study this problem in depth was the Scot David Hulme (1711-66). Hulme provides us with this example:

> The bread which I formerly eat, nourished me; that is a body of such sensible qualities was, at that time, endued with such secret powers: but does it follow, that other bread must also nourish me at another time, and that like sensible qualities must always be attended with like secret powers? The consequence seems nowise necessary (Selby-Bigge and Nidditch, 1975, sect iv, para 29).

Thus it does not necessarily follow from the fact that past bread was nutritious that present or future bread will be. The conjunction of the claims that past bread was nutritious and present or future bread is or will not be nutritious is not a necessary truth. Nevertheless, as Hulme emphasises, we do habitually depend upon our past experience as a guide to the information about the present and future.

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[2]Most of this section drawn freely from Priest's (1990) *The British Empiricists: Hobbs to Ayer* which makes often highly technical and specialised philosophical work accessible to the lay reader.

[3]that any assertion other than the truth is self-contradictory is the basis of the classical definition of a deductive argument

John Stuart Mill (1806-73) defined induction as " ... that operation of the mind by which we infer that what we know in a particular case or cases, will be true in all cases which resemble the former in certain assignable respects" (Mill, 1897, vol 1, p 333). Mill therefore defines induction psychologically, as 'an operation of the mind', but since the work of Frege and Russell it is customary to separate logic from psychology. An important reason for this is any argument is either valid or invalid irrespective of any states of mind, in particular any beliefs about that argument's validity or invalidity. Mill construes the problem of induction as epistemological rather than as a problem in the philosophy of logic. He thinks of it as a question of **how** we can know that what holds true in a restricted number of cases holds true in all cases. Thus the problem might be better construed as whether the truth of the premises of an inductive argument in any sense make true, or make probably true, the conclusion. In Mill's words induction " ... proceeds from the known to the unknown" (Mill, 1897, vol 1, p 333).

Mill maintains that an appeal to the uniformity of nature is powerless to solve the problem of induction because it is itself an inductive principle. Although " that the course of nature is uniform, is the fundamental principle, or general axiom of induction" (Mill, 1897, vol 1, p 355), such a law could only be established by induction, and so produces a circularity in any putative justification of induction employing it as a premise.

Indeed it is not the case that nature is wholly uniform, and it is not the case that inductive generalisations hold true always and everywhere. The unknown does not always resemble the known and the future does not always resemble the past. However, even this distinction between where regularities obtain and where they break down rests on induction: it again being impossible to observe all possible cases of which a generalisation is putatively true.

Bertram Russell (1872-1970) is careful to separate the logical and psychological aspects of induction. That regularity of phenomena is more than mere expectation is illustrated in his example, " the man who has fed the chicken every day throughout its life at last wrings its neck instead, showing that more refined views as to the uniformity of nature would have been useful to the chicken!" (Russell, 1942:58).

Russell concludes that all 'solutions' to the problem of induction must be in the form of a presupposed induction, and so begs the question. Any appeal to the uniformity of nature is circular, because the principle that nature is uniform is the thesis that nature operates in accordance with exceptionless natural laws. This may only be established inductionally, if at all. Any argument that we know the future will resemble the past because past futures have resembled the past is equally inductive, because only induction warrants the belief that future futures will resemble the past.

Russell's contribution is to accept that we cannot logically derive 'all As are B' from 'some As are B' and suggests that the probability of all As being B, or the next A being B is all we should seek. Russell's principle of induction is therefore: the greater the number of cases of A being B, the greater the probability of all As being B, and the greater the number of cases of A being B the greater the probability of the next A being B. It is always more probable that the next A will be a B (other things being equal) than that all As are B, because although 'all As are B' makes 'the next A is B' true, 'the next A is B' does not make 'all As are B' true.

It is claimed that Russell's probabilistic solution of induction is not refuted if, for a particular A, 'A is not B' is true. Suppose we have seen only white swans, in a large and increasing population of swans. Suppose however (as is the case) there is also a population of black swans. We were none the less rational in supposing all swans to be white even though that supposition was false. From the fact that an event is unlikely to occur it does not logically follow that it will not occur, and conversely if an event is likely to occur, it does not logically follow that it will occur (208-10).

A J Ayer (1910-1992) cutely considers the problem of induction to be a 'pseudo-problem' because there is no possible method of solving it. No amount of empirical observation or inspection of meanings will resolve the putative sceptical issue, so "it is a fictitious problem, since all genuine problems are at least capable of being solved" (Ayer, 1976:67). It follows for Ayer therefore that the problem of induction is not a problem for the logic of the natural sciences because induction is not a problem at all!

If we assume the problem is genuine, according to Ayer, there are two and only two, ways of trying to solve it. A claim about the future may be putatively derived from some formal *a priori* claim or from some *a posteriori*, empirical claim, but neither of these is satisfactory. Ayer's view is that all *a priori* propositions are tautologies, but no empirical claim may be derived from a tautology, so no claim about the future may be derived from any *a priori* proposition. Nor on the other hand may any claim about the future be derived from empirical propositions about the past on pain of circularity, because then "one simply assumes what one is setting out to prove" (Ayer, 1976:66). Thus it follows from Ayer's premises that "there is no possible way of solving the problem of induction" (Ayer, 1976:67). Both the *a priori* and *a posteriori* attempts at solution have failed, but as any meaningful statement is either *a priori* or *a posteriori* there remains no meaningful solution to the problem.

The implications of the problem of induction on the existence of knowledge

The problem of induction is clearly crucial not only to scientific method but also to commonsense. The natural laws of the sciences are universal inductive generalisations of the form 'all As are Bs'. They ascribe properties to objects over the whole of space and time, yet any scientist has only sampled some finite portion of the universe and therefore his or her investigations warrant the claim that 'some As are Bs' or 'all the observed so far are Bs'. There is a logical gap between the conclusions justified by the observation and the wide generalisations based on these conclusions.

Induction, on the other hand, is essential to the intelligibility of the world of everyday life. We assume that, as Hulme says, our food will nourish us and not poison us. We assume that when we open the door to leave the room we will step on to a floor and not into total emptiness, that when we stand up we will be able to walk, that we are not about to go blind or deaf, that when we talk to the cat it will not talk back, that the objects about us will stay on the earth and not fly into the air - and so on for thousands and thousands of other beliefs. If it were not for induction our knowledge would be confined at the most to the mere solipsism of the present moment, more likely even the intelligibility of the present moment depends on induction.

Historians give us knowledge of the past based on the present evidence, but it does not follow that 'past As were Bs' from present 'As are Bs' any more than it follows that 'future As are Bs'. The problem of induction opens a gulf in the midst of all our knowledge of the universe. If induction cannot be justified, then none of our knowledge can be justified (157-9).

Induction is presupposed by our science and our commonsense. It is also essential to empiricist philosophy, but, it seems, cannot, be established empirically. The problem of induction cannot be solved: induction is indispensable but unjustifiable.

The twentieth century philosophical view is that each of us is directly acquainted only with our sense data and perhaps ourselves. It follows that knowledge of anything else - the past, the future, other people, indeed anything outside our immediate private experience - must be by other means. Most of our knowledge depends upon induction, on inductive inference from facts about our immediate experience to other facts. This means that the problem of justification of induction is not only a problem in the philosophy of science; it is central to the possibility of nearly all our knowledge (181-3).

Ayer believes that we should have confidence in induction as a practical principle and that this is all that is necessary for the conduct of science. It does not deductively follow from the past explanatory success of science that science will continue to be successful in the future, but it is wrong to hold this as an objection to scientific method because "it is a mistake to demand a guarantee where it is logically impossible to obtain one" (Ayer, 1976:67).

Nor does it follow from the fact that the problem of induction cannot be solved that it is irrational to suppose that the future will resemble the past. It is rational to believe scientific predictions because part of what it is to be rational is to be guided by the past. Scientific method is justified in practice and by experience, not by philosophy. **Scientific prediction, for example, is justified in so far as scientific predictions turn out to be true, and no more than this.** According to Ayer, it is not the role of philosophy to construct *a priori* justifications of scientific method but to remove from its path pseudo-problems of which the problem of induction is one (240-1).

Induction and building economics

What are the implications of the problem of induction for building economics? As a starting point it has to be accepted from the above that the problem of induction is epistemologically devastating. From a common sense point of view, this is really an obvious statement. It is one of the reasons for separating 'beliefs' from 'knowledge'. We may believe that building prices will be 10% more next year than this year, but we cannot know with certainty that this will be the case. Neither can we deduce with certainty that from a sample of say 50 offices projects that all the world's offices projects will exhibit the same underlying economic characteristics. Although both these examples are essentially the same problem - both involve the extrapolation from what is known about our database to what might be the case for similar data not included in our data base - it is convenient to divide this into two issues. Firstly, the uncertainties about the future and secondly the uncertainties about the generalisation from samples to populations. Both these issues are well understood in statistical modelling. In dealing with future events we need to consider the *ex post* errors, and in dealing with samples we need to consider *ex ante* errors or confidence intervals. In these cases, the usual method is to recourse to the use of probability statements to quantify the amounts of uncertainties involved. This is the approach taken by Russell and to which we will return later in considering whether such probability statements are to be treated as beliefs or knowledge.

In building economics as practised by people in industry, eg., quantity surveyors, the treatment of these issues is not so clear. To the best of my knowledge, the problem of induction is not recognised. The demands of practice dictate that a deterministic estimate is given of the project price. From what has been written above, it is clear that this is not logically possible. Perfectly deterministic estimates are logically unattainable in a rational manner. This gives rise to two possibilities, (1) to try to obtain estimates by irrational means or (2) to abandon the search for deterministic estimates. There has not been time or inclination to examine (1) as yet. Instead we will look more closely at (2) and consider the possibilities for nondeterministic estimates.

There are several possible ways of coping with uncertainty in building economics. These range from, in decreasing order of information usage, probabilistic to fuzzy modelling. Thus, in moving away from deterministic estimates, probabilistic estimates are least radical departure. Probabilistic approaches however do demand some assumptions to be made and it could be that a cavalier approach to these assumptions is of no less importance than a cavalier approach to the problem of induction. In this paper considerations will be limited to probabilistic economics and the extent to which they might provide a way around the problem of induction.

Probabilistic economics

As mentioned above, Russell has suggested that, in face of the insurmountable problem of induction, an alternative is to accept the existence of uncertainty and include it in the modelling process. What does this imply?

The first possibility is that by assigning a certain probability to an event we may be just shifting the problem of induction to a different place. If for instance we assign a probability of .01 (1% chance) to the likelihood of the existence of black (or purple striped) swans and it turns out that 10% are actually of this colour, then how would we describe the 'truth' of the probability assignment? Isn't this just a slight of hand brought about by making the dichotomous 'truth' variable into a continuous variable of varying degrees of truth by using the proxy 'degree of belief in truth'? And doesn't this then raise the issue of how good is the estimate of probability and because we don't know the answer to that for certain we have to express it as a probability of a probability and hence into an infinite regress? Should we talk about 'true' (ie unknown) probability values? Is there such a thing and what does it mean? Is it something to do with sampling problems, ie., the bigger the sample the better the estimate of probability? And is an estimate of the proportion of coloured swans in the population just as good as the estimate that the next swan will be coloured?

Most of these questions are concerned with the meaning of probability and Ayer has plenty to say on this subject in a memorable paper in Dowie and Lefrere's (1980) "Risk and Chance". The main point of this is to show that probability is a human concept rather than any real state of nature, and is thus a characteristic of 'belief' rather than 'knowledge'. This is a very important distinction and clarifies much of the confusion implicit in the above questions. For example, a frequency distribution that is compiled from a set of observations on a sample of events is clearly 'knowledge' (ie., factual) about that sample. To make the leap forward and claim that this frequency distribution represents some larger sample yet to be observed is to step out of the factual world in to the world of 'beliefs'. Similarly, to fit a continuous probability distribution to this empirically derived discrete frequency distribution is also a step into the world of beliefs.

On this reasoning, to argue that a probability of 0.01 represents the likelihood of the existence of black swans is somehow connected with the proportion of black swans in the world is to argue that our beliefs about the composition of the world are somehow connected with the real composition of the world. As has been proved, such an argument can only be justified by belief and is therefore circular.

So what then has this to do with truth statements? In what respects can a probability of say, 0.01, be considered to be 'true'? It seems to me that, as probabilities exist only for beliefs, then a probability of 0.01 is a measure of a belief. On this basis it is possible to conceive the existence of a 'true' (but unknown) value of this probability and for which the probability of 0.01 is 'merely' an estimate. Therefore if one was to ask a sample of people for their subjective probability of their being purple striped swans, we might well take the average of these probability 'estimates' as being a close approximation to the 'true' probability of the belief that there may be purple striped swans (assuming that all people believe to the same degree but each is unable to accurately gauge that level of belief).

No doubt none of this is new to the specialists in the philosophy of mathematics, but it is certainly new to the writer and suggests a solution to a problem that has raised its ugly head a few times over the years. The most recent occurrence of this was in the assessment and validation of probabilistic estimates of project cost and times. Using systems such as Bennett and Ormerod's (1984) *Construction Project Simulator*, it is possible to generate a probability distribution of project cost and duration. The question is how to test the generated distribution for accuracy. Clearly, with a system that simply generates point estimates, the accuracy can be measured as the difference or ratio between the forecasted and actual cost or duration. With a probability distribution, it is difficult to see how this may be done. For an actual project cost to lie somewhere within the limits of a probability distribution is not very informative, especially where asymptotic distributions such as the normal or log-normal distributions are involved. Performing this analysis over a series of projects provides more information but still does not enable us to make any clear judgements between competing systems. If the above argument is correct and the probability distribution is only a measure of belief, then the lack of a logical relationship between beliefs and knowledge implies that the real-world status of the probability distributions generated by such systems is empirically untestable. In other words, the probability distribution is not a measure of the likelihood of the bands into which actual costs might fall, but is simply a measure of the uncertainty of belief concerning the value of the actual cost.

The fact that models of the uncertainty of a belief are intrinsically different from the uncertainty of an event also has implications in other belief models such as expert or 'knowledge' based systems. Most of these systems comprise belief models with the uncertainties expressed as uncertainties in beliefs. Again, testing competing systems of this kind is logically limited to matching the model against the beliefs it purports to model. To test the models against actual events is not a test of the model at all but a test of the modelled beliefs. This observation leads to the inevitable conclusion that modelling beliefs is largely a fruitless exercise in the pursuit of knowledge as mismatches between the model and actuality are caused by an inextricable mixture of belief and modelling errors.

Conclusions

The original intention of writing this paper was to introduce the empiricist approach to induction in the context of building economics in order to examine the status of extrapolation from small to large and past to future. The problem of induction has been thoroughly worked over by all the major forces in the field of philosophy over several centuries of thought and the fact that induction is logically indefensible (though of practical value) was established by empiricist philosophers some time ago. As such the subject is somewhat sterile even though it brings into question the whole of empirical research as a knowledge generating activity! That there is no logical basis for believing in the existance of 'laws' of nature or any form of behaviour is a salutory finding and raises important questions concerning the status of so the called 'deterministic' and 'rational' approaches so ingrained in current research. Considering the length of time since this was first established, it is indeed remarkable that so few people criticise the *zeitgeist*.

A secondary issue that is of great interest in building economics is the implications on the relationships between probability, knowledge and beliefs arising from Russell's 'solution'. From the analysis in this paper it is shown that probability can only be a measure of the degree of belief concerning some matter, rather than the degree of knowledge concerning that matter. This is an important result as it specifically denies the possibility of a factual, or factually meaningful, basis to our models and has an important bearing on the way we view, and construct tests on, probabilistic models and 'knowledge' based systems.

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